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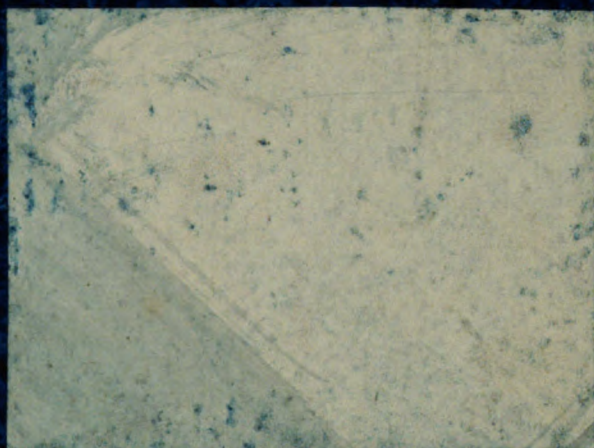
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LETTER

TO

The Right Honourable

GEORGE

EARL OF MACCLESFIELD.

CONCERNING

An Apparent Motion observed in some of the

FIXED STARS.

By JAMES BRADLEY, *D.D.*

Astronomer Royal, *Savilian* Professor of Astronomy at
Oxford, Fellow of the *Royal Society*, and Member of the
Royal Prussian Academy of Sciences and Belles Lettres.

LONDON:

Printed in the Year

M. DCC. XLVII.



A L E T T E R

To the RIGHT HONOURABLE

GEORGE *Earl of* MACCLESFIELD.

My LORD,

THE great Exactness, with which Instruments are now constructed, hath enabled the Astronomers of the present Age to discover several Changes in the Positions of the heavenly Bodies; which, by reason of their *Smallness*, had escaped the Notice of their Predecessors. And altho' the Causes of such Motions have always subsisted, yet Philosophers had not so fully consider'd what the Effects of those known Causes would be, as to demonstrate *a priori* the *Phenomena* they might produce; so that Theory itself is here, as well as in many other Cases, indebted to Practice, for the Discovery of some of its most elegant Deductions. This points out to us the great Advantage of cultivating *this*, as well as every other Branch of Natural Knowledge, by a regular Series of Observations and Experiments.

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The Progress of Astronomy indeed has always been found, to have so great a Dependence upon accurate Observations, that, till such were made, it advanced but slowly : For the first considerable Improvements that it received, in point of Theory, were owing to the renowned *Tycho Brahe*; who far exceeding those that had gone before him, in the Exactness of his Observations, enabled the sagacious *Kepler* to find out some of the principal Laws, relating to the Motion of the heavenly Bodies. The Invention of Telescopes and Pendulum-Clocks affording proper Means of still farther improving the *Praxis* of Astronomy; and these being also soon succeeded by the wonderful Discoveries made by our Great *Newton*, as to its Theory; the Science, in both respects, had acquired such extraordinary Advancement, that future Ages seemed to have little room left, for making any great Improvements. But, in fact, we find the Case to be very different; for, as we advance in the means of making more nice Inquiries, new Points generally offer themselves, that demand our Attention. The Subject of my present Letter to your Lordship, is a Proof of the Truth of this Remark: for, as soon as I had discovered the Cause, and settled the Laws of the Aberrations of the fixed Stars, arising from the Motion of Light, &c. whereof I gave an Account in N^o. 406. of the *Philosophical Transactions*; my Attention was again excited by another new *Phænomenon*; viz. an annual Change of Declination in some of the fixed Stars; which appeared to be sensibly greater about that time, than a Precession of the Equinoctial Points of 50'' in a Year would have occasioned.

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The Quantity of the Difference, thro' small in itself, was rendered perceptible, thro' the Exactness of my Instrument, even in the first Year of my Observations; but being then at a Loss to guess, from what Cause that greater Change of Declination proceeded, I endeavoured to allow for it in my Computations, by making use of the *observed* annual Difference, as mentioned in p. 652. of the same *Transaction*.

. From *that* time to the present, I have continued to make Observations at *Wansted*, as Opportunity offered, with a View of discovering the Laws and Cause of this *Phenomenon*: For, by the Favour of my very kind and worthy Friend *Matthew Wymondesold* Esq, my Instrument has remained where it was first erected; so that I have been able, without any Interruption, which the Removal of it to another Place would have occasioned, to proceed on with my intended Series of Observations, for the Space of twenty Years: a Term somewhat exceeding the whole Period of the Changes, that happen in this *Phenomenon*.

When I shall mention the *small* Quantity of the Deviation, which the Stars are subject to, from the Cause that I have been so long searching after; I am apprehensive, that I may incur the Censure of some Persons, for having spent so much Time in the Pursuit of such a seeming Trifle: But the candid Lovers of Science will, I hope, make due Allowance for that natural Ardour, with which the Mind is urged on towards the Discovery of Truths, in themselves perhaps of *small* Moment, were it not that they tend to illustrate others of greater Use.

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The apparent Motions of the heavenly Bodies are so complicated, and affected by such a Variety of Causes; that in many Cases it is extremely difficult to assign to each its due Share of Influence; or distinctly to point out, what Part of the Motion is the Effect of one Cause, and what of another: And whilst the joint Effects of *All* are only attended to, great Irregularities and seeming Inconsistencies frequently occur; whereas, when we are able to allot to each particular Cause its proper Effect, Harmony and Uniformity usually ensue.

Such seeming Irregularities being also blended with the unavoidable Errors, which Astronomical Observations must be always liable to, as well from the Imperfection of our Senses, as of the Instruments that we make use of) have often very much perplex'd those, who have attempted to solve the *Phænomena*: and till Means are discovered, whereby we can separate and distinguish the *particular* Part of the whole Motion, that is owing to each respective Cause, it will be impossible, to be well assured of the Truth of any Solution. For these Reasons, we generally find, that the more exact the Instruments are, that we make use of, and the more regular the Series of Observations is, that we take; the sooner we are enabled to discover the Cause of any new *Phænomenon*. For when we can be well assured of the Limits, wherein the Errors of the Observations are contain'd; and have reduced them within as narrow Bounds as possible, by the Perfection of the Instruments which we employ; we need not hesitate to ascribe such apparent Changes, as manifestly exceed those Limits, to some other Causes. Upon these

these Accounts it is incumbent upon the *practical* Astronomer, to set out at first with the Examination of the Correctness of his Instruments; and to be assured that they are sufficiently exact for the Use he intends to make of them: or at least he should know, within what Limits their Errors are confined.

This Practice has, in an eminent manner, been lately recommended by your Lordship's noble Example; who having, out of a singular Regard for the Science of Astronomy, erected an Observatory, and furnished it with as complete an *Apparatus* of Instruments, as our best Artists could contrive; would not fully rely on their Exactness, till their Divisions had undergone the strictest Re-examination: whereby they are probably now render'd as perfect in their kind, as any extant, or as human Skill can at present produce.

The Lovers of *this* Science in general, cannot but acknowledge their Obligations to your Lordship on this Account; but I find myself more particularly bound to do it; since, by means of your Lordship's most accurate Observations, I have been enabled to settle some *principal Elements*; which I could not at present otherwise have done, for want of an Instrument at the Royal Observatory, *proper* for that Purpose: For the large *mural Quadrant*, which is there fixed to observe Objects lying Southward of the Zenith, however *perfect* an Instrument it may be in it self, is not *alone* sufficient to determine, with proper Exactness, either the *Latitude* of the Observatory, or the Quantity of Refraction corresponding to different Altitudes: For it being too heavy to be conveniently removed; and the Room wherein it is placed, being too small to admit of its bein

being turned to the opposite Side of the Wall, whereon it now hangs; I cannot, by *actual* Observations of the circumpolar Stars, settle those necessary Points; and therefore have endeavoured to do it, by comparing my own with your Lordship's Observations: and until this Defect in the *Apparatus* belonging to the Royal Observatory be removed, we must be indebted to your Lordship, for the Knowledge of its true Situation.

A Mind intent upon the Pursuit of any kind of Knowledge, will always be agreeably entertained, with what can supply the most proper means of attaining it: Such, to the practical Astronomer, are exact and well-contriv'd Instruments; And I reflect with Pleasure on the Opportunities I have enjoyed, of cultivating an Acquaintance and Friendship with the Person, that, of all others, has most contributed to their Improvement. For I am sensible, that if my own Endeavours have, in any respect, been effectual to the Advancement of Astronomy; it has principally been owing to the Advice and Assistance given me by our worthy Member Mr. *George Graham*; whose great Skill and Judgment in Mechanicks, join'd with a complete and practical Knowledge of the Uses of Astronomical Instruments, enable him to contrive and execute them in the most perfect manner.

The Gentlemen of the *Royal Academy of Sciences*, to whom we are so highly obliged for their exact Admeasurement of the Quantity of a Degree under the Arctic Circle, have already given the World very convincing Proofs of *his* Care and Abilities in those Respects; and the particular Delineation, which they have lately published, of the several Parts
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of the *Sector*, which he made for them, hath now rendered it needless, to enter upon any minute Description of *mine* at *Wansted*; both being constructed upon the same Principles, and differing in their component Parts, chiefly on account of the different Purposes, for which they were intended.

As mine was originally designed to take only the *Differences* of the Zenith Distances of Stars, in the various Seasons of the Year, without any View of discovering their *true* Places; I had no Occasion to know exactly, what Point on the Limb corresponded to the *true* Zenith: and therefore no Provision was made in my Sector, for the changing of its Situation for that Purpose. Neither was it necessary that the Divisions or Points on the Arc should be set off, with the utmost Accuracy, Equidistant from each other, because, when I observe any particular Star, the same Spot or Point being first bisected by the Plumb-line, and then the Screw of the Micrometer turn'd until the Star appears upon the middle of the Wire, that is fixed in the common Focus of the Glasses of the Telescope; I can thereby collect, how far the Star is from that given Point at the Time of Observation: and afterwards, by comparing together the several Observations that are made of it, I am able to discover what apparent Change has happen'd. The Quantity of the visible Alteration, in the Position of the Stars, being expressed by Revolutions and Parts of a Revolution, of the Screw of the Micrometer; I endeavoured to determine, with great Care, the true Angle answering thereto: and after various Trials, I thoroughly satisfied myself, both of the Equality of the

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the Threads of the Screw, and of the precise Number of Seconds corresponding to them.

But altho' these Points could be settled with great Certainty, I was nevertheless obliged to make one Supposition; which perhaps to some Persons may seem of too great Moment in the present Inquiry, to be admitted without an evident Proof from Facts and Experiments. For I *suppose*, that the Line of Collimation of my Telescope has invariably preserved the same Direction, with respect to the Divisions upon the Arc, during the whole Course of my Observations. And indeed it was on account of the Objections, which might have been raised against such a *Postulate*, that I thought it necessary, to continue my Series of Observations for so many Years, before I published the Conclusions; which I shall at present endeavour to draw from them.

Whoever compares the Result of the several Trials, that have been made by the Gentlemen of the *Academy of Sciences*, for determining the Zenith Point of their Sector, since their Return from the North; will, I presume, allow that *mine* is not an unreasonable or precarious *Supposition*: since it is evident, from their Observations, that the Line of Collimation of that Instrument underwent no sensible Change in its Direction, during the Space of more than a whole Year; altho' it was several times taken down, and set up again in different and remote Places; whereas *mine* hath always remained suspended in the same Place.

But besides such a strong Argument for the Probability of the Truth of my *Supposition*, I have the Satisfaction of finding it *actually* verified by the Observations

Observations themselves; which plainly prove, that at the End of the full Period of the Deviations which I am going to mention, the Stars are found to have the same Positions by the Instrument, as they ought to *have*, supposing the Line of Collimation to have continued unaltered from the Time when I first began to observe.

I have already taken notice, in what manner this *Phænomenon* discover'd itself to me at the End of my first Year's Observations, *viz.* by a *greater* apparent Change of Declination in the Stars near the Equinoctial Colure, than could arise from a Precession of 50'' in a Year; the mean Quantity now usually allowed by Astronomers. But there appearing at the same time, an Effect of a quite contrary Nature, in some Stars near the Solstitial Colure, which seem'd to alter their Declination *less* than a Precession of 50'' required; I was thereby convinced, that all the *Phænomena*, in the different Stars, could not be accounted for, merely by supposing, that I had assumed a wrong Quantity for the Precession of the Equinoctial Points.

At first, I had a Suspicion, that some of these small apparent Alterations in the Places of the Stars, might possibly be occasioned by a Change, in the Materials, or in the Position of the Parts of my Sector: But, upon considering how firmly the Arc, on which the Divisions or Points are made, is fastened to the Plate, wherein the Wire is fixed that lies in the Focus of the Object-Glass; I saw no Reason to apprehend, that any Change could have happened in the Position of that Wire and those Points. The Suspension therefore of the Plummets being the most likely Cause, from whence I conceived any Uncer-

tainty could arise; and the Wire of which had been broken three or four times in the first Year of my Observations: I attempted to examine, whether Part of the foremention'd apparent Motions might not have been owing, to the different Plumb-lines that had been made use of. In order to determine this, I adjusted a particular Point of the Arc to the Plumb-line, with all the Exactness I could; and then taking off the old Wire, I immediately hung on another, with which the same Spot was again compared. I repeated the Experiment three or four times, and thereby fully satisfied myself, that no sensible Error could arise from the Use of different Plumb-lines; since the various Adjustments of the same Point agreed with each other, within less than half a Second.

Having then, from such Trials, sufficient Reason to conclude, that these *second* unexpected Deviations of the Stars, were not owing to any Imperfection of my Instrument; after I had settled the Laws of the Aberrations arising from the Motion of Light, &c. I judged it proper to continue my Observations of the same Stars; hoping that, by a regular and longer Series of them, carried on thro' several succeeding Years, I might, at length, be enabled to discover the *real* Cause of such apparent Inconsistencies.

As I resided chiefly at *Wansted*, after my Sector was erected there in the Year 1727. till the Beginning of *May* 1732. when I removed from thence to *Oxford*: I had, during my Abode at *Wansted*, frequent Opportunities of repeating my Observations; and thereby discovered so many Particulars relating

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to these *Phænomena*, that I began to guess what was the real Cause of them.

It appeared from my Observations, that, during this Interval of Time, some of the Stars near the Solstitial Colure, had changed their Declinations 9'' or 10'' *less*, than a Precession of 50'' would have produced; and, at the same time, that, others near the Equinoctial Colure, had altered theirs about the same Quantity *more*, than a like Precession would have occasioned: the North Pole of the Equator seeming to have approached the Stars, which come to the Meridian with the Sun, about the Vernal Equinox and the Winter Solstice; and to have receded from those, which come to the Meridian with the Sun, about the Autumnal Equinox and the Summer Solstice.

When I consider'd these Circumstances, and the Situation of the Ascending Node of the Moon's Orbit, at the time when I first began my Observations; I suspected, that the Moon's Action upon the Equatorial Parts of the Earth might produce these Effects: For, if the Precession of the Equinox be, according to Sir *Isaac Newton's* Principles, caused by the Actions of the Sun and Moon upon those Parts; the Plane of the Moon's Orbit being at *one* time, above ten Degrees *more* inclined to the Plane of the Equator, than at *another*; it was reasonable to conclude, that the Part of the whole annual Precession, which arises from her Action, would in different Years be varied in its Quantity; whereas the Plane of the Ecliptic, wherein the Sun appears, keeping always nearly the same Inclination to the Equator; *that* Part of the Precession, which is owing to the Sun's Action, may be the same every Year:

Year: And from hence it would follow, that, altho' the *mean* annual Precession; proceeding from the joint Actions of the Sun and Moon, were $50''$; yet the *apparent* annual Precession might sometimes exceed, and sometimes fall short, of that mean Quantity, according to the various Situations of the Nodes of the Moon's Orbit.

In the Year 1727. when my Instrument was first set up, the Moon's Ascending Node was near the Beginning of *Aries*; and consequently, her Orbit was as much inclined to the Equator, as it can at any time be; and then the *apparent* annual Precession was found, by my first Year's Observations, to be greater than the *mean*: which proved, that the Stars near the Equinoctial Colure, whose Declinations are most of all affected by the Precession, had changed *theirs*, above a tenth Part more than a Precession of $50''$ would have caused. The succeeding Years Observations proved the same Thing; and in three or four Years time the Difference became so considerable, as to leave no Room to suspect, that it was owing to any Imperfection, either of the Instrument or Observations.

But some of the Stars, which I had observed, that were near the Solstitial Colure, having appeared to move, during the same time, in a manner contrary to what they ought to have done, by an Increase in the Precession; and the Deviations in them being as remarkable as in the others, I perceived that something more, than a mere Change in the Quantity of the Precession, would be requisite to solve this Part of the *Phænomenon*. Upon comparing my Observations of Stars near the Solstitial Colure, that were almost

almost opposite to each other in Right Ascension, I found, that they were equally affected by this Cause; for whilst γ *Draconis* appeared to have moved Northward, the small Star, which is the 35th *Camelopardali Hevel.* in the *British Catalogue*, seem'd to have gone as much towards the South: which shew'd, that this apparent Motion, in both those Stars, might proceed from a Nutation in the Earth's Axis; whereas the Comparison of my Observations of the same Stars, *formerly* enabled me to draw a different Conclusion, with respect to the Cause of the annual Aberrations arising from the Motion of Light. For the apparent Alteration in γ *Draconis*, from *that* Cause, being as great again as in the other small Star, proved, that *that Phenomenon* did not proceed from a *Nutation* of the Earth's Axis; as, on the contrary, *this* may. Upon making the like Comparison between the Observations of other Stars, that lie nearly opposite in Right Ascension, whatever their Situations were with respect to the Cardinal Points of the Equator, it appeared; that their Change of Declination was nearly equal, but contrary; and such as a Nutation or Motion of the Earth's Axis would effect.

The Moon's Ascending Node being got back towards the Beginning of *Capricorn* in the Year 1732: the Stars near the Equinoctial Colure appeared, about that time, to change their Declinations no more, than a Precession of 50'' required; whilst some of those near the Solstitial Colure altered *theirs* above 2'' in a Year less, than they ought. Soon after, I perceived the annual Change of Declination of the former to be diminished, so as to become *less* than
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50'' of Precession would cause; and it continued to diminish till the Year 1736. when the Moon's Ascending Node was about the Beginning of *Libra*, and her Orbit had the *least* Inclination to the Equator. But by this time, some of the Stars near the Solstitial Colure had altered their Declinations 18'' *less*, since the Year 1727. than they ought to have done from a Precession of 50''. For γ *Draconis*, which in those nine Years should have gone about 8'' more *Southerly*, was observed in 1736. to appear 10'' more *Northerly*, than it did in the Year 1727.

As this Appearance in γ *Draconis*, indicated a Diminution of the Inclination of the Earth's Axis to the Plane of the Ecliptic; and as several Astronomers have supposed *that* Inclination to diminish regularly; if this *Phænomenon* depended upon such a Cause, and amounted to 18'' in nine Years, the Obliquity of the Ecliptic would, at that rate, alter a whole Minute in thirty Years; which is much faster than any Observations, *before* made, would allow. I had Reason therefore to think, that *some Part* of this Motion at the least, if not the *Whole*, was owing to the Moon's Action upon the Equatorial Parts of the Earth; which I conceived, might cause a libratory Motion of the Earth's Axis. But as I was unable to judge, from only nine Years Observations, whether the Axis would entirely recover the same Position, that it had in the Year 1727. I found it necessary to continue my Observations thro' a whole Period of the Moon's Nodes; at the End of which I had the Satisfaction to see, that the Stars returned into the same Positions again; as if there had been no Alteration at all in the Inclination of the Earth's Axis: which

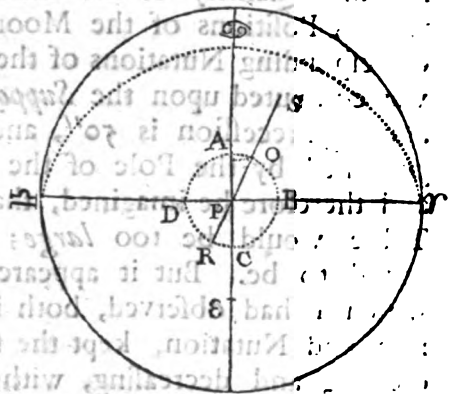
which fully convinced me, that I had guessed rightly as to the Cause of the *Phænomena*. This Circumstance proves likewise, that if there be a gradual Diminution of the Obliquity of the Ecliptic; it does not arise only from an Alteration in the Position of the Earth's Axis, but rather from some Change in the Plane of the Ecliptic itself: because the Stars, at the End of the Period of the Moon's Nodes, appeared in the same Places, with respect to the Equator, as they ought to have done, if the Earth's Axis had retained the same Inclination to an invariable Plane.

During the Course of my Observations, our ingenious Secretary of the *Royal Society*, Mr. *John Machin*, being employed in considering the Theory of Gravity; and its Consequences, with regard to the Celestial Motions; I acquainted him with the *Phænomena* that I had observed: and at the same time mentioned, *what* I suspected to be the Cause of them. He soon after sent me a Table, containing the Quantity of the annual Precession in the various Positions of the Moon's Nodes, as also the corresponding Nutations of the Earth's Axis; which was computed upon the *Supposition*, that the *mean* annual Precession is $50''$, and that the Whole is governed by the Pole of the Moon's Orbit only: and therefore he imagined, that the Numbers in the Table would be too *large*; as in Fact they were found to be. But it appeared, that the Changes which I had observed, both in the annual Precession and Nutation, kept the same Law, as to increasing and decreasing, with the Numbers of his Table. Those were calculated upon the *Supposition*,
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that the Pole of the Equator, during a Period of the Moon's Nodes, moved round in the Periphery of a little Circle, whose Center was $23^{\circ} 29'$ distant from the Pole of the Ecliptic; having itself also an angular Motion of $50''$ in a Year, about the same Pole: The North Pole of the Equator was conceived to be in *that* Part of the small Circle, which is farthest from the North Pole of the Ecliptic, at the Time when the Moon's Ascending Node is in the Beginning of *Aries*: and in the opposite Point of it, when the same Node is in *Libra*.

Such a Hypothesis will account for an Acceleration and Retardation of the annual Precession; as also for a Nutation of the Earth's Axis: And if the Diameter of the little Circle be supposed equal to $18''$; which is the whole Quantity of the Nutation, as collected from my Observations of γ *Draconis*: then all the *Phænomena* in the several Stars which I observed, will be very nearly solved by it.

Let *P* represent the mean Place of the Pole of the Equator, about which Point, as a Center, suppose the true Pole to move in the Circle *ABCD*, whose Diameter is $18''$. Let *E* be the Pole of the Ecliptic, and *EP* be equal to the mean Distance between the Poles of the Equator and Ecliptic; and suppose the true Pole



Pole of the Equator to be at *A*, when the Moon's Ascending Node is in the Beginning of *Aries*; and at *B*, when the Node gets back to *Capricorn*; and at *C*, when the same Node is in *Libra*: at which time the North Pole of the Equator being nearer the North Pole of the Ecliptic, by the whole Diameter of the little Circle *AC* equal to $18''$; the Obliquity of the Ecliptic will then be so much *less* than it was, when the Moon's Ascending Node was in *Aries*. The Point *P* is supposed to move round *E*, with an equal retrograde Motion, answerable to the mean Precession arising from the joint Actions of the Sun and Moon: while the true Pole of the Equator moves round *P*, in the Circumference *ABCD*, with a retrograde Motion likewise, in a Period of the Moon's Nodes, or of eighteen Years, and seven Months. By this means, when the Moon's Ascending Node is in *Aries*, and the true Pole of the Equator at *A*, is moving from *A* towards *B*: it will approach the Stars, that come to the Meridian with the Sun about the Vernal Equinox; and recede from those that come with the Sun near the Autumnal Equinox, *faster* than the *mean* Pole *P* does. So that, while the Moon's Node goes back from *Aries* to *Capricorn*, the *apparent* Precession will seem so much *greater* than the *mean*; as to cause the Stars, that lie in the Equinoctial Colure, to have altered their Declination $9''$, in about four Years and eight Months, *more* than the mean Precession would do: and in the same time, the North Pole of the Equator will seem to have approached the Stars, that come to the Meridian with the Sun at our Winter Solstice, about $9''$; and to have receded as *much* from those, that come with the Sun at the Summer-Solstice.

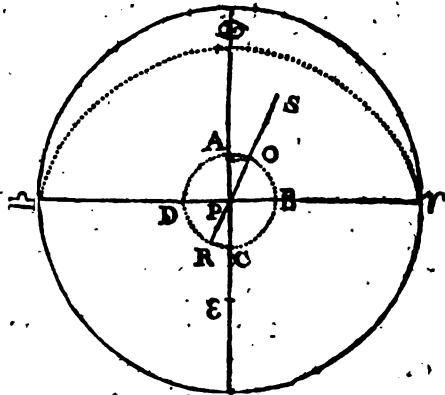
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Thus the *Phænomena* before recited are in general conformable to this Hypothesis. But to be more particular; let

S be the Place of a Star, PS the Circle of Declination passing thro' it, representing its Distance from the mean Pole, and γPS its mean Right Ascension.

Then if O and R be the Points, where the Circle of Declination cuts the



little Circle $ABCD$; the *true* Pole will be nearest that Star at O , and farthest from it at R ; the whole Difference amounting to $18''$, or to the Diameter of the little Circle. As the true Pole of the Equator is supposed to be at A , when the Moon's Ascending Node is in *Aries*; and at B , when that Node gets back to *Capricorn*; and the angular Motion of the true Pole about P , is likewise supposed equal to that of the Moon's Node about E , or the Pole of the Ecliptic: since, in these Cases, the true Pole of the Equator is 90° Degrees before the Moon's Ascending Node; it must be so in all others.

When the true Pole is at A , it will be at the same Distance from the Stars that lie in the Equinoctial Colure, as the mean Pole P is; for I neglect at present the Case of such Stars as are *very* near the Pole of the Equator; and as the true Pole recedes back from A towards B , it will approach the Stars, that lie in that Part of the Colure represented by Pr ; and recede from those, that lie in $P \perp$; not indeed

indeed with an *equable* Motion; but in the *Ratio* of the *Sine* of the Distance of the Moon's Node from the Beginning of *Aries*. For if the Node be supposed to have gone backwards from *Aries* 30° , or to the Beginning of *Pisces*; the Point, which represents the Place of the true Pole, will in the mean time, have moved in the little Circle, thro' an Arc, as *AO*, of 30° likewise: and would therefore in Effect have approached the Stars that lie in the Equinoctial Colure *Pv*, and have receded from those that lie in *P* \approx $4\frac{1}{2}$; which is the *Sine* of 30° to the *Radius AP*. For if a Perpendicular fall from *O* upon *PA*, it may be conceived as Part of a great Circle, passing thro' the true Pole and any Star lying in the Equinoctial Colure. Now the same Proportion, that holds in these Stars, will obtain likewise in all others; and from hence we may collect a general Rule, for finding how much nearer or farther, any particular Star is, to or from, the *mean* Pole, in any given Position of the Moon's Node.

For, if from the *Right-Ascension* of the Star, we subtract the *Distance* of the Moon's *Ascending Node* from *Aries*; then the *Radius* will be to the *Sine* of the *Remainder*, as $9''$, is to the *Number of Seconds*, that the Star is nearer to, or farther from the *True*, than the *Mean Pole*. When that Remainder is less than 180° , the Star is nearer to the *True*, than to the *Mean Pole*; and the contrary, when it is greater than 180° .

This Motion of the *true* Pole, about the *mean* at *P*, will also produce a Change in the *Right Ascensions* of the Stars, and in the Places of the *Equinoctial Points*; as well as in the *Obliquity* of the *Ecliptic*.

tic: and the Quantity of the Equations, in either of these Cases, may be easily computed for any given Position of the Moon's Nodes. But as it may be needless, to dwell longer on the Explication of the Hypothesis; I shall now proceed to shew its Correspondency with the *Phænomena*, relating to the Alterations of the Polar Distances of some of the Stars which I have observed: by laying before your Lordship the Observations themselves, together with the Computations that are necessary; in order to form a right Judgment about the Cause of these Appearances!

I have endeavoured to find the exact Quantity of the *mean* Precession of the Equinoctial Points, by comparing my own Observations made at *Greenwich*, with those of *Tycho Brabè* and others, which I judged to be most proper for that Purpose. But as many of the Stars, which I compared, gave a different Quantity; I shall assume the mean Result; which gives a Precession of one Degree in seventy-one Years and an half: this agreeing very well likewise with my Observations that were taken at *Wansted*. The Numbers in the following Tables, which express the Change of Declination in each Star, are computed upon the Supposition, that the *mean* Obliquity of the Ecliptic was $23^{\circ}. 28'. 30''$, and that it continued the *same*, during the whole Course of my Observations. And as the Moon's Ascending Node was in the Beginning of *Aries* about the 27th Day of *March* 1727, I have reduced the Place of each Star to *that* Time; by allowing the proper Change of Declination from that Day, to the Day of each respective Observation.

It being also necessary to make an Allowance for the *Aberrations* of Light; I have again examined
my

my Observations, that were most proper to determine the Transverse Axis of the Ellipsis, which each Star seems to describe; and have found it to be nearest to $40''$; which Number I therefore make use of in the following Computations.

The Divisions or Points upon the Limb of my Sector are placed five Minutes of a Degree from each other; and are numbered so, as to shew the Polar Distances nearly; the *true* Polar Distance exceeding that, which is shewn by the Instrument, about $1'.35''$. When I first began to observe, I generally made use of *that* Point on the Limb, which was nearest to the Star's Polar Distance, without regarding whether it was more Northerly, or more Southerly than the Star; but as it sometimes happened, that the Original Point, with which I at first compared the Star, became, in Process of Time, pretty remote from it; I afterwards brought the Plummet to another Point, that was nearer to it; and carefully examined, what Number of Revolutions of the Screw of the Micro-meter &c. corresponded to the Distance between the different Points; that I had made use of: by which means I was able to reduce all the Observations of the same Star to the same Point, without supposing the several Divisions to be accurately $5'$ asunder.

I have expressed the Distance of each Star from the Point of the Arc, with which it was compared, in *Seconds* of a Degree and *tenth Parts* of a Second, exactly as it was collected from the Observations; altho' I am sensible, that the Observations themselves are liable to an Error of more than a *whole* Second; because I meet with some, that have been made within two or three Days of each other, that differ $2''$, even when they are not marked as *defective* in any respect. It

It would be too tedious, to set down the whole Number of the Observations that I have made; and therefore I shall give only enough of them, to shew their Correspondency with the forementioned Hypothesis in the several Years, wherein any were made of the Stars here recited. When *several* Observations have been taken of the same Star, within a few Days of each other; I have either set down the mean Result, or *that* Observation which best agreed with it. I have likewise commonly chosen those, that were made near the same Season of the Year, in such Stars as gave me the Opportunity of making that Choice; particularly in γ *Draconis*, which was generally observed about the End of *August* or the Beginning of *September*; That being the usual Time, when I went to *Wansted* on purpose to observe both *that*, and also some of the Stars in the *great Bear*. But the Weather proving cloudy at that Season in the Year 1744, prevented my making a single Observation, either of γ *Draconis*, or any other Star, while I was there; which is the Cause of one Vacancy in a Series of 20 succeeding Years, wherein that particular Star had been observed. Such Stars, as were either not visible in the Day-time, towards the Beginning of *September*, or came at such Hours of the Night, as would have incommoded the Family of the House wherein the Instrument is fixed, were but seldom observed, after I went to reside at *Oxford*: which is the Reason, why the Series of Observations of *those* is so imperfect, as sometimes to leave a Chasm for several Years together. But notwithstanding this, I doubt not, but upon the whole they will be found sufficient, to satisfy

satisfie your Lordship of the general Correspondency between the *Hypothesis* and the *Phænomena*, in the several Stars; however different their Situations are, with respect to the Cardinal Points of the Equator.

As I made more Observations of γ *Draconis* than of any other Star; and it being likewise very near the Zenith of *Wansted*; I will begin with the Recital of some of them. The Point upon the Limb, with which this Star was compared, was $38^{\circ}. 25'$ from the North Pole of the Equator, according to the Numbers of the Arc of my *Sector*. The first Column, in the following Table, shews the Year and the Day of the Month, when the Observations were made; the next gives the Number of *Seconds*, that the Star was found to be *South* of $38^{\circ}. 25'$: the third contains the Alterations of the Polar Distance, which the *mean* Precession, at the rate of one Degree in $71\frac{1}{2}$ Years, would cause in this Star, from the 27th Day of *March* 1727, to the Day on which the Observation was taken: the fourth shews the Aberrations of Light: the fifth, the Equations arising from the 'forementioned Hypothesis: and the sixth gives the *mean* Distance of the Star from the Point with which it was compared, found, by collecting the several Numbers, according to their Signs, in the 3d, 4th, and 5th Columns, and applying them to the *Observed Distances* contain'd in the Second.

If the Observations had been perfectly exact, and the several Equations of their *due* Quantity; then all the Numbers in the last Column would have been equal; but since they differ a little from one another; if the *mean* of All be taken, and the Extremes are

D compared

pared with it, we shall find no greater Difference, than what may be supposed to arise from the Uncertainty of the Observations themselves; it no where amounting to more than $1''\frac{1}{2}$. The Hypothesis therefore seems, in this Star, to agree extremely well with the Observations here set down; but as I had made above 300 of it; I took the Trouble of comparing each of them with the Hypothesis: and altho' it might have been expected, that, in so large a Number, some great Errors would have occurred; yet there are very few, *viz.* only eleven, that differ from the mean of these so much as $2''$; and not one that differs so much as $3''$. This surprising Agreement, therefore, in so long a Series of Observations, taken in all the various Seasons of the Year, as well as in the different Positions of the Moon's Nodes, seems to be a sufficient Proof of the Truth, both of *this* Hypothesis, and also of *that* which I formerly advanced, relating to the Aberrations of Light; since the Polar Distance in this Star may differ, in certain Circumstances, almost a Minute, *viz.* $56''\frac{1}{2}$, if the Corrections resulting from both these Hypotheses are neglected; whereas, when those Equations are rightly applied, the mean Place of the Star comes out the same, as nearly, as can be reasonably expected.

γ Draconis

γ <i>Draconis</i>		South of 0 38. 25	Preces- sion.	Aberra- tion.	Nuta- tion.	Mean Dist.
		"	"	"	"	"
1727	September	3 70.5	— 0.4	+ 19.2	— 8.9	80.4
1728	March	18 108.7	— 0.8	— 19.0	— 8.6	80.3
	September	6 70.2	— 1.2	+ 19.3	— 8.1	80.2
1729	March	6 108.3	— 1.6	— 19.3	— 7.4	80.0
	September	8 69.4	— 2.1	+ 19.3	— 6.4	80.2
1730	September	8 68.0	— 2.9	+ 19.3	— 3.9	80.5
1731	September	8 66.0	— 3.8	+ 19.3	— 1.0	80.5
1732	September	6 64.3	— 4.6	+ 19.3	+ 2.0	81.0
1733	August	29 60.8	— 5.4	+ 19.0	+ 4.8	79.2
1734	August	11 62.3	— 6.2	+ 16.9	+ 6.9	79.9
1735	September	10 60.0	— 7.1	+ 19.3	+ 7.9	80.1
1736	September	9 59.3	— 8.0	+ 19.3	+ 9.0	79.5
1737	September	6 60.8	— 8.8	+ 19.3	+ 8.5	79.8
1738	September	13 62.0	— 9.6	+ 19.3	+ 7.0	78.7
1739	September	2 66.6	— 10.5	+ 19.2	+ 4.7	80.0
1740	September	5 70.8	— 11.3	+ 19.3	+ 1.9	80.7
1741	September	2 75.4	— 12.1	+ 19.2	— 1.1	81.4
1742	September	5 76.7	— 12.9	+ 19.3	— 4.0	79.1
1743	September	2 81.6	— 13.7	+ 19.1	— 6.4	80.6
1745	September	3 86.3	— 15.4	+ 19.2	— 8.9	81.2
1746	September	17 86.5	— 16.2	+ 19.2	— 8.7	80.8
1747	September	2 86.1	— 17.0	+ 19.2	— 7.6	80.7

I made about 250 Observations of β *Draconis*; which I find correspond as well with the Hypothesis, as those of γ ; but since the Positions of both these Stars, in respect to the Solstitial Colure, differ but little from each other; it will be needless to set down the Observations of β . I shall therefore proceed to lay before your Lordship, some Observations of a small Star, that is almost opposite to γ *Draconis*

conis in Right Ascension, being the 35th *Camelopardali Hevel.* in the *British Catalogue*. Mr. *Flamsteed*, indeed, has not given the Right Ascension of this Star; but *that* being necessary to be known, in order to compute the Change of its Declination arising from the Precession of the Equinox; I compared the Time of its Transit over the Meridian, with that of some other Stars near the same Parallel; whereby I found, that its Right Ascension was $85^{\circ}. 54.\frac{1}{2}$ at the Beginning of the Year 1737.

This small Star was compared with the same Point of the Limb of my Sector, as γ *Draconis*; and the second Column, in the following Table, shews how many *Seconds* it was found to be South of that Point, at the time of each respective Observation. The other Columns contain, as in the foregoing Table, the Equations that are necessary to find, what its *mean* Distance from the same Point would have been on the 27th Day of *March* 1727, which is exhibited in the last Column. The whole Number of my Observations of this Star did not much exceed forty; the greatest Part of which were made before the Year 1730; in some of the following Years none were taken; and only a single one in any other, except in 1739. However, their Correspondency seems sufficient to evince the Truth of the Hypothesis: for if the Mean of these, contain'd in the Table, be taken, not one, among the rest of the Observations, will differ from it more than $2''$.

35 th <i>Camelopard. Hevelii.</i>		South of 0° 25'	Precel- sion.	Aberra- tion.	Nutation.	Mean Diff. South.
		" "	" "	" "	" "	" "
1727	October 20	73.6	+ 0.9	- 6.7	+ 8.9	76.7
1728	January 12	60.8	1.2	+ 6.1	8.8	76.9
	March 1	57.8	1.4	+ 9.4	8.7	77.3
	September 26	75.2	2.3	- 8.8	8.1	76.8
1729	February 26	56.4	2.8	+ 9.4	7.6	76.2
1730	March 3	57.8	4.4	9.4	5.4	77.0
1731	February 5	59.1	5.6	8.5	+ 3.0	76.2
1733	January 31	64.1	8.7	8.2	- 2.9	78.1
1738	December 30	61.8	17.2	4.3	6.5	76.8
1739	February 4	56.9	17.3	8.5	6.3	76.4
1740	January 20	56.0	18.6	7.0	- 4.0	77.6
1747	February 27	32.3	28.5	9.4	+ 8.4	78.6

The Observations of the foregoing Stars are the most proper, to prove the Change of the Inclination of the Earth's Axis to the Plane of the Ecliptic; those, which follow, will shew in what manner the Stars, that lie near the Equinoctial Colure, are affected, as well as others, that are differently situated, with respect to the Cardinal Points of the Equator. Some of these Stars are indeed more remote from the Zenith, than I would have chosen, if there had been others, of equal Lustre, in more proper Positions; because Experience has long since taught me, that the Observations of such Stars, as lie near the Zenith, do generally agree best with one another, and are therefore the fittest to prove the Truth of any Hypothesis. I shall begin with those near the Vernal Equinox. α *Cassiopeæ* was compared with the Point marked $34^{\circ} 55'$; and at first

was

was found to be more *Southerly*, but afterwards became more *Northerly* than that Point, as in the following Table; the last Column of which shews its mean Distance *South* of that Point on the 27th of *March* 1727. The Observation of the 23d Day of *December*, in the Year 1738, differs 3'' from the *mean* of the others; as does also another, that was taken five Days after this; neither of which being marked as uncertain, I judged it proper to insert one of them; altho' they give the mean Place of the Star near 2 Seconds more *Northerly* than any other, in a Series of above 100; *all* of which correspond, with the *mean* of these here recited, within less than 2''; excepting *two*, that give the Stars mean Distance almost 3'' more *Southerly*; but these last mentioned are marked as dubious; and indeed they appear to have been bad, by comparing them with several others, that, were made near the same time, from which they differ almost 2''.

<i>α Cassiopeæ.</i>	South of ° ' "	Precel- sion.	Aberra- tion.	Nuta- tion.	Mean Dist. South
	34. 55	"	"	"	"
1727 September 9	55.0	+ 9.0	+ 2.2	+ 2.4	68.6
1728 September 17	30.8	29.4	+ 4.6	5.2	70.0
1729 June 8	35.7	43.8	— 16.3	6.8	70.0
December 3	N. 0.4	53.5	+ 16.5	7.7	68.3
1730 June 11	S. 13.8	64.0	— 16.2	8.4	70.0
December 9	N. 30.8	73.8	+ 16.3	8.8	68.1
1732 January 8	N. 49.2	95.4	12.9	8.9	68.0
1733 January 21	64.8	116.0	+ 10.0	7.9	69.1
1734 June 13	62.8	143.8	— 16.1	5.0	69.9
December 11	105.4	153.7	+ 16.2	+ 3.7	68.2
1738. December 23	176.3	234.0	+ 15.2	— 7.2	65.7
1740 June 2	169.1	262.8	— 16.5	— 8.9	68.3
1747 February 27	332.3	397.0	+ 0.2	+ 4.7	69.6

Altho'

Altho' I have taken no Observation of τ *Persei* since the 22d Day of *January* 1740; yet, as this Star is very near the Zenith, and a sufficient Number were made about the Times when the Equation, resulting from the Hypothesis, was at its *Maximum*; I judged it proper to insert some of them in the next Table; the last Column of which shews, how much the Star's *mean* Distance was *South* of $38^{\circ} 20'$ on the 27th Day of *March* 1727. Among near 60 Observations I meet with two only, that differ from the mean of these so much as $2''$; and those differ almost as much from the mean of others, that were taken near the same time: so that the Hypothesis seems to correspond, in general, with the Observations of this Star as well, as with either of the foregoing.

τ <i>Persei</i> .	South of ° 38. 20	Precel- sion.	Aberra- tion.	Nuta- tion.	Mean Dist. South.
	" "	" "	" "	" "	" "
1727 September 16	60.1	+ 7.4	- 3.2	+ 6.7	71.0
December 29	39.7	11.9	+ 12.9	7.2	71.7
1728 December 21	22.5	27.2	12.8	8.7	71.2
December 28	9.2	42.0	11.5	9.0	71.7
1731 January 3	N. 8.2	59.0	12.8	8.3	71.9
1732 January 8	22.0	74.8	12.7	6.7	72.2
1733 January 21	34.6	91.0	11.7	+ 4.3	72.4
1738 December 23	117.0	183.4	12.8	- 9.0	70.2
1740 January 22	132.5	200.2	11.7	8.6	70.8

After the last recited Observations, it may perhaps seem needless to add those of α *Persei*, which is farther from the Zenith; but however, as this Star lies very nearly at an equal Distance from the Equinoctial

Equinoctial and Solstitial Colures, and the Series of Observations of it is somewhat more complete, than that of τ *Persei*; I shall insert one at least, for each Year wherein it has been observed; whereby it may appear, that the Hypothesis solves the *Phænomena* of Stars in this Situation, as exactly as in others: for if a *mean* be taken of the Numbers in the last Column of the following Table, which expresses the *mean* Distance of the Star *South* of $41^{\circ} 5'$. on *March 27th 1727*, it will agree within two Seconds, with every one of 80 Observations, that have been made of this Stat.

α <i>Persei</i>	South of ° 41. 5	Preces- sion.	Aberra- tion.	Nutation	Mean Dist. South.
	"	"	"	"	"
1727 December 29	79.4	+ 10.5	+ 11.4	+ 7.9	109.2
1728 April 7	87.5	14.3	— 0.8	8.2	109.2
July 5	94.6	17.7	— 11.4	8.5	109.4
December 13	65.7	23.8	+ 10.6	8.8	108.9
1729 December 3	53.4	37.2	9.7	8.9	109.2
1731 January 2	38.6	52.3	11.4	7.8	110.1
1732 January 8	26.8	66.2	+ 11.4	+ 5.9	110.3
1734 July 11 S.	21.3	101.0	— 11.4	— 1.1	109.8
1738 December 24 N.	56.3	162.6	+ 11.2	9.0	108.5
1740 January 21	71.8	177.4	10.9	— 8.2	108.3
1747 February 27	182.5	275.4	6.6	+ 8.5	108.0

Having already given Examples of Stars, lying near both the Solstices and the *Vernal* Equinox; I shall now add the Observations of *one*, that is not far from the *Autumnal* Equinox, *viz.* α *Ursæ Majoris*, the brightest Star in that Part of the Heavens, which approaches the Zenith of *Wansted* within a Degree; and

and which, by reason of its Lustre and Position, gave me the Opportunity of making my Series of Observations of *It*, more complete than of many others. This Star was compared with the Point marked $39^{\circ} 15'$ and was *South* of it as in the following Table; wherein your Lordship will see, that the Observations of the Years 1740 and 1741 give the Polar Distances $3''$ greater, than the *mean* of the other Years. Had there been only a single Observation taken in either of those Years, Part of this apparent Difference might have been supposed to arise from their Uncertainty; but as there were 8 Observations taken within a Week, either before or after the 3d Day of *June* 1740, which agree well with each other; and three were made within 20 Days in *September* 1741, which likewise corresponded with each other; I am inclined to think, that the 'foremention'd Differences must be owing to something else, besides the Error of the Observations. This *Phenomenon* therefore may deserve the Consideration of those Gentlemen, who have employed their Time in making Computations relating to the Quantity of the Effects, which the Power of Gravity may, on various Occasions, produce. For I suspect, that the Position of the Moon's Apogee, as well as of her Nodes, has some Relation to the apparent Motions of the Stars that I am now speaking of.

My Series of Observations of several Stars abound, of late Years, with so many and long Interruptions; that I cannot pretend to *determine* this Point; but probably the Differences before taken notice of in the Observations of *a Cassiopeæ*, and some others

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that I have found likewise among the Observations of *other* Stars, that are not here recited, may be owing to such a Cause; which, altho' it should not have any large Share of Influence, may yet, in certain Circumstances, discover a Defect in a Hypothesis, that pays no Regard at all to *It*. But whether these Differences do arise from the Cause already hinted at; or whether they proceed from any Defect of the Hypothesis itself in any other respect; it will not be very *material* in point of Practice; since *that* Hypothesis, as it was before laid down, appears to be sufficient to solve all the *Phænomena*, to as great a Degree of Exactness, as we can in general *hope* or *expect* to make Observations. For if I take the *mean* of all the Numbers in the last Column of the following Table for *γ Ursæ Majoris*, and compare it with any one of 164 Observations that were taken of it, the Difference will not exceed three Seconds.

<i>γ Ursæ Majoris</i>		South of 0°	Precession.	Aberration.	Nutation	Mean Diff. South.
		39. 15				
1727	October 17	153.3	— 10.2	+ 1.0	— 5.2	138.9
1728	January 24	176.4	15.2	— 17.6	5.8	137.8
	July 17	150.8	23.9	+ 17.8	6.9	137.8
	October 11	176.6	28.2	+ 2.6	7.3	137.7
1729	January 16	196.6	33.1	— 17.8	7.8	137.9
	July 21	170.4	42.4	+ 17.8	8.4	137.4
1730	July 19	189.6	60.6	+ 17.8	9.0	137.8
	December 28	232.4	68.7	— 16.7	8.9	138.1
1734	September 18	218.1	81.9	+ 9.4	8.4	137.2
1732	January 10	250.7	87.7	— 17.7	8.0	137.3
	April 12	238.7	92.3	— 0.8	7.7	137.9
1734	July 11	255.7	133.3	+ 17.6	— 2.3	137.7

γ Ursæ

<i>n Ursæ Majoris</i>		South of	Preces-	Aberra-	Nutation	Mean
		° ' "	sion.	tion.		Dist.
		39. 15				South
		"	"	"	"	"
1735	September	10 280.8	154.6	+ 11.4	+ 1.2	138.8
1736	September	8 294.7	172.8	11.6	4.1	137.6
1737	July	3 303.0	187.8	17.2	6.1	138.5
1738	June	29 319.0	205.8	16.8	7.9	137.9
1739	April	25 348.0	220.8	2.5	8.8	138.5
1740	June	3 360.3	241.1	12.8	8.9	140.9
1741	September	23 390.9	265.0	7.9	+ 7.4	141.2
1745	September	5 466.7	337.1	12.4	- 3.3	138.7
1746	September	20 492.0	356.2	8.8	5.9	138.7
1747	September	25 507.2	373.5	13.2	7.8	139.1

You may perceive, my Lord, by inspecting the Tables which contain the Observations of α *Cassiopeæ* and *n Ursæ Majoris*; that the greatest Differences that occur therein may be diminished, by supposing the *true* Pole of the Equator to move round the Point *P*, in an *Ellipsis*, instead of a Circle. For if the transverse Axis, lying in the Direction *AC*, be 18", and the Conjugate, as *DB*, be about 16"; the Equations, resulting from such an Hypothesis, will make the Numbers in the last Columns agree with each other, nearer than as they now stand. But since this would not entirely remove the Inequalities, in all the Positions of the Moon's Nodes; I shall refer the more accurate Determination of the *Locus* of the *true* Pole to Theory; and at present only give the Equations for the Precession of the Equinoctial Points, and the Obliquity of the Ecliptic, as also the real Quantity of the annual Precession, to every 5th Degree of the Place of the Moon's Ascending Node, in the following Tables; just as

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they

they result from the Hypothesis, as at first laid down;
it appearing, from what has already been remark'd,
that these will be sufficiently exact for Practice in all
Cases.

The Equation of the Equinoct. Points						The Equation of the Obliquity of the Ecliptick.					
Ds \odot from γ	Sig. O	I	II	Subit		Ds \odot from γ	Sig. U	I	II	Add	
	Sig. V	VII	VIII	Add			Sig. VI	VII	VIII	Subit	
0	"	"	"	0		0	"	"	"	0	
5	0.0	1 3	19 6	30		5	9.0	7.8	4.5	20	
10	2.0	13 0	20.5	25		10	9.0	7.4	3.8	25	
15	3.9	14 5	21.2	20		15	8.9	6.9	3.1	20	
20	5.8	6.0	21 8	15		20	8.7	6.4	2.3	15	
25	7.7	17.3	22.2	10		25	8.5	5.8	1.6	10	
30	9.6	18.5	22.5	5		30	8.2	5.2	0.8	5	
Subit.	11 3	19.6	22.6	0		Subit.	7.8	4.5	0.0	0	
Add	Sig. V	IV	III	Ds \odot from γ		Add	Sig. V	IV	III	Ds \odot from γ	
	Sig. XI	X	IX				Sig. XI	X	IX		

The Annual Precession of the Equinoctial Points.							
Ds \odot from γ	Sig. O	I	II	III	IV	V	
0	"	"	"	"	"	"	0
5	58.0	57.0	54.2	50.3	46.5	43.7	30
10	57.9	56.6	53.6	49.7	46.0	43.4	25
15	57.9	56.2	53.0	49.0	45.5	43.2	20
20	57.7	55.7	52.3	48.4	45.0	43.0	15
25	57.5	55.2	51.7	47.7	44.5	42.8	10
30	57.3	54.7	51.0	47.1	44.1	42.8	5
	57.0	54.2	50.3	46.5	43.7	42.7	0
	Sig. XI	X	IX	VIII	VII	VI	Ds \odot from γ

Sir

Sir *Isaac Newton*, in determining the Quantity of the annual Precession from the Theory of Gravity, upon Supposition that the Equatorial is to the Polar Diameter of the Earth as 230 is to 229, finds the Sun's Action sufficient to produce a Precession of $9''\frac{1}{2}$ only; and, collecting from the Tides the Proportion between the Sun's Force and the Moon's to be as 1 to $4\frac{1}{2}$, he settles the mean Precession, resulting from their joint Actions, at $50''$. But since the Difference between the Polar and Equatorial Diameter is found, by the late Observations of the Gentlemen of the *Academy of Sciences*, to be greater than what Sir *Isaac* had computed it to be; the Precession, arising from the Sun's Action, must likewise be greater than what he has stated it at, nearly in the same Proportion. From whence it will follow, that the Moon's Force must bear a less Proportion to the Sun's than $4\frac{1}{2}$ to 1; and perhaps the *Phænomena*, which I have now been giving an Account of, will supply the best *Data* for settling this Matter.

As I apprehend, that the Observations already set down will be judged sufficient, to prove in general the Truth of the Hypothesis before advanced; I shall not trouble your Lordship with the Recital of more, that I made of Stars lying at greater Distances from the Zenith; those not being so proper, for the Reason before-mention'd, to establish the Point that I had chiefly in View. But as it may perhaps be of some Use to future Astronomers, to know what were the *mean* Differences of Declination, at a given Time, between some Stars, that lie nearly opposite to one another in Right Ascension, and not far from either of the *Colures*; I shall set down the Result of the Comparison of a few, that differ so little in Declination, that

that I could determine the Quantity of that Difference with great Certainty.

By the *mean* of 64 Observations, that were made of α *Cassiopeæ* before the End of the Year 1728, I collect, after allowing for the Precession, Aberration and Nutation as in the foregoing Tables; that the *mean* Distance of this Star was $68''.7$ South of $34^{\circ}.55'$, on the 27th Day of *March* 1727. By a like Comparison of 40 Observations, taken of γ *Ursæ Majoris* during the same Interval of Time, I find this Star was, at the same time, $39''.6$ South of $34^{\circ}.45'$. I carefully measured, with the Screw of the Micrometer, the Distance between the Points, with which these Stars were compared; and found them to be $9'.59''$ from each other, or one Second less than they ought to have been. Hence it follows, that the *mean* Difference of Declination between these two Stars, was $10'.28''.1$, on the 27th Day of *March* 1727.

By the *mean* of 65 Observations, that were taken of β *Cassiopeæ*, before the End of the Year 1728, this Star was $25''.8$ North of $32^{\circ}.20'$, on the 27th Day of *March* 1727: and by the *mean* of 52 Observations, ϵ *Ursæ Majoris* was $87''.6$ South of $32^{\circ}.30'$ at the same time. The Distance between these Points was found to be $9'.59''.3$; from whence it follows, that the *mean* Difference of Declination between these two Stars was $11'.52''.7$ on *March* 27th 1727.

By the *mean* of 100 Observations, taken before the End of the Year 1728, the *mean* Distance of γ *Draconis* was $79''.8$ South of $38^{\circ}.25'$ on *March* 27th 1727; and by the *mean* of 35 Observations, the

the 35th *Camelopard. Hevel.* was South of the same Spot $76''.4$. So that the mean Polar Distance of γ *Draconis* was only $3''.4$ greater, than that of the 35th *Camelopard. Hevel.* but as the Equation for the Nutation, in both these Stars, was then near the *Maximum*, and to be applied with contrary Signs; the *Apparent* Polar Distance of γ *Draconis* was $21''.4$ greater, on the 27th Day of *March* 1727.

The Differences of the Polar Distances of the Stars, as here set down, may be presumed, both on account of the Radius of the Instrument and the Number of Observations, to be very exactly determined, to the Time when the Moon's Ascending Node was at the Beginning of *Aries*; and if a like Comparison be hereafter made, of Observations taken of the same Stars, near the same Position of the Moon's Nodes; future Astronomers may be enabled, to settle the Quantity of the mean Precession of the Equinox, so far as it affects the Declination of these Stars, with great Certainty; and they may likewise discover, by means of the Stars near the Solstitial Colure, from what Cause the apparent Change in the Obliquity of the Ecliptic really proceeds, if the mean Obliquity be found to diminish gradually.

The forementioned Points indeed can be settled only on the Supposition, that the angular Distances of these Stars do continue always the same, or that they have no real Motion in themselves; but are at Rest in Absolute Space. A Supposition, which though usually made by Astronomers, nevertheless seems to be founded on too uncertain Principles, to be admitted in all Cases. For if a Judgment may be formed, with Regard to this Matter, from the Result

sult of the Comparison of our best modern Observations, with such as were formerly made with any tolerable Degree of Exactness; there appears to have been a real Change in the Position of some of the fixed Stars, with respect to each other; and such, as seems independent of any Motion in our own System, and can only be referred to some Motion in the Stars themselves. *Arcturus* affords a strong Proof of this: for if its present Declination be compared with its Place, as determined either by *Tycho* or *Flamsteed*; the Difference will be found to be much greater, than what can be suspected to arise from the Uncertainty of their Observations.

It is reasonable to expect, that other Instances of the like kind must also occur among the great Number of the visible Stars: because their relative Positions may be alter'd by various means. For if our own Solar System be conceived to change its Place, with respect to Absolute Space; this might, in Process of Time, occasion an apparent Change in the angular Distances of the fixed Stars; and in such a Case, the Places of the nearest Stars being more affected, than of those that are very remote; their relative Positions might seem to alter; tho' the Stars themselves were really immoveable. And on the other Hand, if our own System be at Rest, and any of the Stars really in Motion, this might likewise vary their apparent Positions; and the more so, the nearer they are to us, or the swifter their Motions are, or the more proper the Direction of the Motion is, to be rendered perceptible by us. Since then the Relative Places of the Stars may be changed from such a Variety of Causes, considering that amazing Distance

tance at which it is certain some of them are placed, it may require the Observations of many Ages, to determine the Laws of the apparent Changes, even of a single Star; much more difficult therefore must it be, to settle the Laws relating to all the most remarkable Stars.

When the Causes, which affect the Places of all the Stars in general are known; such as the Precession, Aberration, and Nutation; it may be of singular Use, to examine nicely the relative Situations of particular Stars: and especially of those of the greatest Lustre, which, it may be presumed lie nearest to us; and may therefore be subject to more sensible Changes; either from their own Motion, or from that of our System. And if at the same time that the brighter Stars are compared with each other, we likewise determine the relative Positions of some of the *smallest* that appear near them, whose Places can be ascertained with sufficient Exactness; we may perhaps be able to judge to what Cause the Change, if any be observable, is owing. The Uncertainty that we are at present under, with respect to the Degree of Accuracy wherewith former Astronomers could observe, makes us unable to determine several Things, relating to the Subject that I am now speaking of: but the Improvements, which have of late Years been made in the Methods of taking the Places of the heavenly Bodies, are so great, that a few Years may hereafter be sufficient, to settle some Points; which cannot now be settled, by comparing even the earliest Observations with those of the present Age.

It were to be wish'd therefore, that such Persons as are provided with proper Instruments, would attempt to determine, with great Care, the present relative Positions of several of the Principal Stars, in various Parts of the Heavens, especially of those, that are least affected by Refraction: *that* Cause having many times so uncertain an Influence on the Places of Objects, that are very remote from the Zenith; that wherever *It* is concerned, the Conclusions, deduced from Observations that are *much* affected by it, will always remain doubtful, and too precarious, in many Cases, to be relied upon.

The Advantages, arising from different Persons attempting to settle the same Points of Astronomy near the same time, are so much the greater; as a Concurrence in the Result, would remove all Suspicion of Incorrectness in the Instruments made use of. For which Reason, I esteem the curious *Apparatus* at *Shirburn Castle*, and the Observations there taken, as a most valuable *Criterion*, whereby I may judge of the Accuracy of those, that are made at the *Royal Observatory*: and as a Lover of Science I cannot but wish, that our Nation abounded with more frequent Examples, of Persons of like Rank and Ability with your Lordship, equally desirous of promoting *This*, as well as every other Branch of Natural Knowledge, that tends to the Honour and Benefit of our Country.

But were the Patrons of Arts and Sciences ever so numerous, the Subject of my present Letter is of such a Nature, as must direct me, to beg Leave to address it to the *Earl of Macclesfield*; not only as a most competent Judge of it; but as the *sole* Person,

son, in this Nation, that hath Instruments proper to examine into the Truth of the Facts here related. And it is a particular Satisfaction to me, that after so long an Attendance upon these *Phænomena*, I am allowed the Honour of transmitting the Account of them to the Public, thro' your Lordship's Hands: as it gives me at the same time an Opportunity of professing the grateful Sense I shall ever retain, both of the signal Favours which I formerly received from the noble Earl your Father, and of the many recent Obligations conferr'd by yourself upon,

My LORD,

Your Lordship's

most obedient

humble Servant,

*Greenwich, Dec. 31.
1747.*

Ja. Bradley.

